gations: chest x-ray normal; Hb 55%; W.B.C. 7,100 with normal differential; iron deficiency on the blood film; E.S.R. 138 mm. in one hour; Paul–Bunnell test negative. Blood culture revealed *Haemophilus influenzae* type B sensitive to all the usual antibiotics except sulphonamide. This child was treated with ampicillin and made a good recovery.

My point in recording this case history is to emphasize that septicaemia due to *Haemophilus influenzae* may not be as rare as we had thought and one can easily envisage such a child as I have mentioned developing osteomyelitis due to this organism.—I am, etc.,

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Monoamine Oxidase Inhibitors and Anaesthesia

SIR,—The risks involved in anaesthetizing patients receiving monoamine oxidase inhibitors have been largely eliminated in the West Dorset Group of Hospitals by the adoption of the following routine for admission of cases from the surgical waiting-list.

(1) Every patient is instructed to bring with him any pills, medicines, etc., he is currently receiving.

(2) The patient's general practitioner is sent a form several days before the day of admission informing him of the proposed admission. This form is prestamped and returnable, and contains the following questionary:

"What drugs is the patient currently receiving? If the patient has received steroids during the past two years or monoamine oxidase inhibitors during the past two weeks please give details."

This procedure has been in operation for nearly two years and has proved to be very valuable, thanks to the co-operation of the general practitioners in the area.—I am, etc.,

J. W. WARRICK.

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Idiopathic Facial Palsy

SIR,—Mr. John Groves (24 February, p. 508) has performed a valuable service in drawing attention to the possibility that lack of properly matched controls vitiates the conclusion of Dr. D. Taverner and others (9 December 1967, p. 581) that A.C.T.H. is of proved benefit in the treatment of Bell's palsy. They say that they are convinced of its efficacy, but the fallibility of personal conviction or clinical impression is too well known to warrant further comment. Moreover, in that part of their previous trial where properly matched controls were used no significant difference between the treated and the untreated groups was shown.

The observation of Mr. Groves, that as one sees cases in hospital series earlier in the course of Bell's palsy so does the denervation rate lessen, is confirmed by the following figures in a series of 128 cases of complete unilateral Bell's palsy referred for electrodiagnostic testing. These cases were seen on the first occasion at any time up to 28 days from the onset of the condition, and the denervation rate (the criteria for denervation being those described by Taverner *et al.*[1]) was 40%. If out of this series those seen up to 21 days are studied then the denervation rate drops to 33%. In cases up to 14 days the denervation rate is 29%, up to 7 days 20%, and if those seen only up to 5 days are selected the rate is 13%.

None of those patients had any treatment designed to prevent denervation, and this denervation rate of 13% is the same as in Taverner's five-day series, in which those patients predicted to be in danger of denervation had been treated with A.C.T.H. Thus it becomes clear that a five-day treated series can be compared with a concurrent five-day untreated series only to give a valid comparison, whereas Taverner and his colleagues have compared a five-day treated series retrospectively with a 14-day untreated, thus giving a built-in bias of considerable magnitude in favour of the method of treatment. This method of comparison amply illustrates the dictum quoted by Bradford Hill[2]: "The assessment of therapeutic activity by the use of retrospective controls is an inherently fallacious method."

So far the criticisms and comments of Mr. Groves have remained unanswered—perhaps because they are unanswerable.—I am, etc.,

London W.1. E. D. R. CAMPBELL.

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Zinc Sulphate and Bedsores

SIR,—Acceleration of the healing time of open wounds in a small group of young men who were given zinc sulphate 220 mg. three times daily by mouth has been reported,[1] with no undesirable side-effects. The same treatment was given to six elderly people who had bedsores at the time of admission to a geriatric unit.

Case 1.—80-year-old man with a bedsore on the right heel, 6.5 cm. by 6 cm. by 5 cm.; full thickness loss of skin; present at least one month; no sign of healing. Zinc sulphate 220 mg. given three times daily for 33 days was associated with complete healing and new epithelium.

Case 2.—73-year-old man with diabetes who had a perforating ulcer on sole of right foot, 5 cm. by 2 cm.; full thickness loss of skin, and present "for a long time." Zinc sulphate was given three times daily for 27 days, after which partial healing was observed but patient insisted on going home. Fifty days later the patient was seen again. Treatment had probably been discontinued, but there remained only three "islands" of broken skin, 1 cm., 1 cm., and 0.5 cm. in diameter.

Case 3.—70-year-old man with a bedsore on his right heal, 6 cm. by 6 cm. by 5 cm.; full thickness loss of skin and involvement of underlying tissues; present for at least four months. Zinc sulphate three times daily produced no effect for one month, after which healing began and became complete in 106 days.

Case 4.—70-year-old woman with bedsore on her right heel, 5 cm. by 4 cm., full thickness loss of skin, and present for at least three months. Zinc sulphate was given three times daily for 66 days, when complete healing was noted.

Case 5.—81-year-old woman with a superficial ulcer over the sacral region and bedsores on both heels, with full thickness loss of skin at the left heel. Zinc sulphate was given three times daily for 17 days, when both heels were noted to be healed.

Case 6.—84-year-old woman developed a deep sacral sore 8 cm. by 6 cm. with a track at least 0.5 cm. diameter into underlying tissue during treatment for a perforated gall bladder. The ulcer showed no sign of healing after 12 months, during which time energetic treatment included two surgical toilet operations. Zinc sulphate was given twice daily with no effect for about one month, after which healing was noted, and at the moment there is residual superficial loss of skin 2 cm. by 0.5 cm.

Pories *et al.*[1] indicated that "zinc is the metal moiety in a number of essential enzymes," and that "zinc is preferentially concentrated in healing tissues with a peak activity in the first days after injury." Zinc sulphate was given in the above cases in addition to standard treatment for bedsores, and control cases were not included. Experienced members of the nursing staff thought that healing was promoted, the final stage of healing was not delayed compared with previous treatment regimens, and "new skin" appeared over the ulcer.

No ill effects have been seen in the six cases, and, though the initial results are encouraging, further evaluation of the effectiveness of zinc sulphate, the minimum dose required, and the mode of action is required. It is hoped that the above cases will be reported in more detail elsewhere.—I am, etc.,

Stracathro Hospital, Brechin, Angus. CYRIL COHEN.

REFERENCE

1 Pories, W. J., Henzel, J. H., Rob, C. G., and Strain, W. H., *Lancet*, 1967, 1, 121.

Paracervical Block with Bupivacaine

SIR,—I have read with interest Mr. D. H. Gudgeon's article (18 May, p. 403).

I have just completed a series of 100 cases of paracervical block with bupivacaine 0.5% with adrenaline 1:200,000. My initial experience with 0.25% was the same as Mr. Gudgeon's in that the duration of analgesia was about three hours. I did not feel that this was sufficiently long, and have found that the higher dosage gives a much improved duration of effect. We have, however, noted the occurrence of severe foetal bradycardia on a few occasions, and the occurrence of this complication has never been satisfactorily explained, but from the work of other authors adrenaline does not appear to be implicated. I feel that if Mr. Gudgeon's series had been rather longer foetal bradycardia might well have been observed.

It is hoped that our results will shortly be published.—I am, etc.,

F. C. R. PICTON.

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Cirrhosis of the Liver

SIR,—I was interested to read the article on cirrhosis of the liver by Dr. A. E. Read (17 February, p. 427), as it concisely and lucidly presented the generally accepted British views on hepatic cirrhosis. Certain differences stand out in marked contrast with the disease seen in India, where it is no less common. They are as follows.

Alcohol in the aetiology of disease can be totally ruled out. One hardly ever comes across a cirrhotic who has ever consumed alcohol. Cirrhosis in adults is also far more common in Hindu males who are traditionally vegetarian. They come from a poorer socio-

economic strata and are often residents of rural areas. Diet lacks high-class proteins and consists mostly of cereals and a small quantity of pulses. Very low levels of total serum proteins and albumin are noted in hospitalized patients. The presumptive evidence of protein malnutrition and deprivation is strong, unlike the cryptogenic types in Britain.

Less than a quarter of patients give a history of jaundice. Presuming that all jaundice in the past is due to infectious hepatitis, the actual percentage may be much less, as pallor of anaemia due to ankylostomiasis is often wrongly described as icterus by these patients, as they are not good witnesses of their past events. Further, infectious hepatitis is more common in cities, but cirrhotics are mostly drawn from villages. It is therefore likely that viral hepatitis may be of even lesser importance in causation of cirrhosis in India than in England.

A majority of patients, 55 to 75%, show a post-necrotic pattern of hepatic tissue.—I am, etc.,

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Folate Requirement in Pregnancy

SIR,—The detailed investigations reported by Dr. I. Chanarin and others (18 May, pp. 390 and 394) are clearly of great importance in further defining the pathogenesis of folic-acid deficiency in pregnancy as an equation of supply and demand. We have recently completed a somewhat similar investigation where serial whole-blood folate levels were followed throughout pregnancy and the puerperium in patients either on iron alone or on iron plus 330 μg. of folic acid a day. Few low whole-blood or red-cell folate values were found at the time of delivery in the group on iron alone, but by six weeks postpartum a sharp fall had occurred, with the median whole-blood folate value for the group lying below the lower limit of our normal controls but within the range found in overt megaloblastic anaemia.

We have interpreted this unexpected finding as being due to the delay with which the red-cell (or whole-blood) folate levels reflect the folate stores, this in turn being due to the stability of the intra-erythrocytic folate and the prolonged survival of erythrocytes in the circulation, as pointed out by Dr. Chanarin and by others.[1] Tests of the red-cell folate at 38 weeks, as by Dr. Chanarin's group, or of the whole-blood folate at 37–39 weeks, as by Hansen and Rybo,[2] are therefore likely to reflect the average state of folate balance pertaining over the preceding three months (or 110 days).

The finding of much lower levels at six weeks after delivery suggested to us that maximum folate depletion usually occurred in the last few weeks of pregnancy and in the early post-partum period. This would not be unreasonable if foetal growth, blood-loss at delivery, and lactation were important factors in maternal folate utilization. The finding of a normal red-cell folate at 37–39 weeks by Hansen and Rybo[2] and at 38 weeks by Chanarin *et al.* after supplementation by 100 μg. a day may indicate that this dose is adequate to meet requirements up to 30 or 32 weeks' gestation while still leaving it an open question whether this dose will always prove adequate over the period of delivery and the puerperium.

Our previous investigation, utilizing the post-partum serum folate level,[3] and the more recent work, utilizing the six-week post-partum whole-blood folate level, suggest that in the Glasgow population 300 μg. a day is closer to the maximum requirements at the later stages of pregnancy. This dose, however, appears to be in excess of daily requirements during the earlier stages of pregnancy, since excessively high red-cell folate levels were found at term on this dose. As explained in your leading article (18 May, p. 377), a larger prophylactic dose is required for those patients with unusually low dietary intakes, and this fact may also introduce regional variations in apparent requirements as judged by this type of investigation.

I must also point out that the reference in Dr. Chanarin's paper to whole-blood folate levels in patients on various folic acid supplements found by me[4] is unfortunately erroneous. The whole-blood folate levels reported in my paper (Table III) referred to tests on the 6-weeks-old babies of mothers on these supplements. It is true that a maternal supplement of 100 μg. a day caused no detectable elevation of the babies' whole-blood folate, but that a very slight elevation was found when this was 300 μg. a day.—I am, etc.,

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Low-dosage Oral Progestogen

SIR,—Continuous low-dosage progestogen therapy is a current subject for research into "contraceptive pill" preparations, as exemplified by the paper from Dr. J. Zañartu and others (4 May, p. 263). So that the records of the subject can be kept in order I draw attention to Table II in the above article (p. 264).

The following calculations are based on the data given in the paper:

(1) There was a total of 24 pregnancies in 3,400 observed months.

$$= \frac{24 \text{ pregnancies in 3,400 months}}{3{,}400} \text{ pregnancies per 1 woman month}$$

$$= \frac{24 \times 12}{3{,}400} \text{ pregnancies per 1 woman year}$$

$$= \frac{24 \times 12 \times 100}{3{,}400} \text{ pregnancies per 100 woman years}$$

= 8.5 pregnancies per 100 woman years, not 6.1 as inferred in Table II.

(2) If one considers the 11 pregnancies which occurred due to omission of the pills a similar calculation gives 3.9 pregnancies per 100 woman years, not 2.8 as noted in Table II.

(3) The 13 pregnancies due to method failure can be calculated to be equivalent to 4.6 pregnancies per 100 woman years, not 3.3 as noted in Table II.

Indeed, the title of Table II is also somewhat misleading in an article about the inhibition of fertility with a progestogen. It would seem more appropriate to entitle the Table "Failure of Antifertility Effect" rather than "Recovery of Fertility." These are of course minor points of little importance to the general content of the paper, but they are nevertheless pertinent.—I am, etc.,

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Brand Names

SIR,—With reference to your leading article (30 March, p. 781), any reasonable proposal likely to decrease the multiplicity of drug names and to facilitate the identification of pharmaceutical preparations is most welcome. However, usage of generic names would be more common if they were less cumbersome and therefore more easily remembered. Compare, for instance, the generic name ethyl biscoumacetate with its trade equivalent Tromexan, or phenoxybenzamine hydrochloride with Dibenzyline, or diphenhydramine hydrochloride with Benadryl. Surely an aspect in the choice of generic names should be the consideration whether they can be conveniently prescribed in a busy practice.

If it is not always easy to find the generic names which correspond to British and American brand names the problem is much greater with the foreign-language literature, where lists of brand names are often not readily available if they exist at all. This makes it difficult or impossible to understand articles in western, and still more so in eastern, European journals. It would be a great step forward if editors of journals agreed to include in all papers the generic names of drugs mentioned as adopted in, say, the U.S. and in one of the western European countries. When a generic name is not available the chemical formula should be given.—I am, etc.,

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Gender and Sex

SIR,—Of course I agree with Dr. D. H. Smyth (11 May, p. 368) that the words "sex" and "gender" are not facultatively interchangeable. But even the grammarians are, or were, not always consistent. When Latin was a *sine qua non* for entry into a medical school some of us must have chanted from Allen's *Latin Grammar:* "Common are to either sex, *artifex* and *opifex*. . . ." I never knew, nor do I now, the meaning of *opifex*.—I am, etc.,

A. McKIE REID.
Liverpool.

*** *Opifex* (from *opus facere*, to do work) means "workman" or, presumably, "working-woman."—ED., *B.M.J.*

SIR,—I sympathize with Professor D. H. Smyth's complaint (11 May, p. 368) about my phrase "young of both genders"; "of both sexes" would have been more conventional, but not necessarily more correct. For he is wrong in suggesting that the word gender "was invented by grammarians." The *Oxford English Dictionary* gives its first